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January 23, 2017

To: Director, U.S. Geological Survey
12201 Sunrise Valley Drive
MS 101 National Center
Reston, VA 20192

From: Andrew C. Weaver, PE, CFM
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Dear Director,

I am seeking an appeal of a USGS decision on a previously submitted information correction request which I submitted on February 10, 2016. The response from USGS was dated January 4, 2017. Although the decision was to decline my main request, USGS staff did agree to revise three flows (1974, 1975, and 1977) for which I am grateful.

I do have a few comments/suggestions about the decision, which I've included on the following pages. I respectfully request that you reconsider your decision, but regardless of the outcome I appreciate everyone's time, expertise, and feedback regarding my request, and thank USGS staff for your efforts.

Regards,

Andrew C. Weaver

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Andrew C. Weaver, PE, CFM, mASCE

Comment 1: I request that USGS reconsider the decision based on Rantz et.al. (1982), Volume 2. At the very least I request a note on the main web page of the gage https://waterdata.usgs.gov/pa/nwis/uv/?site_no=01576500&PARAmeter_cd=00065,00060,00010 that explains how the Viaduct downstream of the gage was changed in 1990.

Due to the construction of East Walnut Street through the Conestoga River Viaduct (causing an approximate 20% reduction in cross section), there was a greater than 10% error in the 1974, 1975, and 1978 flows so I thank USGS for noticing this and revising the flows using the correct rating curve. According to the January 4, 2017 decision, none of the other stages or flows had more than a 10% error so nothing else will be revised. There is generally a good reason for this policy because "stage-discharge relations are usually subject to minor random fluctuations resulting from the dynamic force of moving water" (Rantz et.al, 1982 p. 345), and a 10% cutoff seems reasonable. This is based on the assumption that some amount of error in measurement is unavoidable, random, and measurement error will tend to balance out over the long run.

The present rating curve, which passes through both the 2011 and the 1972 peaks, appears to diverge from the guidance in Rantz, et. al. (1982), Volume 2, in three ways:

1) Rantz, page 346 - "`In the U.S.A. if the random departure of a discharge measurement from a defined segment of the rating curve is within +/- 5 percent of the discharge value indicated by the rating, the measurement is considered to be a verification of the rating curve. If several consecutive measurements meet the 5-percent criterion, but they all plot on the same side of the defined segment of the rating curve, they may be considered to define a period of shifting control.`" Assuming all data from gage 01576500 is given equal weight, the present rating curve is based on 61 years (1929 - 1990) of obsolete annual peaks in which the "error" is mostly on one side of the rating curve, and only 26 years (1990 - 2016) of current annual peaks mostly on the other side of the curve. A large part of the "error" now seen in the pre-1990 data is due to a known change (construction of East Walnut Street), not unavoidable random measurement error. It seems very likely that the larger number of pre-1990 observations is pulling the curve away from the newer, more "correct" data.

2) Rantz, page 348 - "`The feeling in the USA is that more weight in the analysis should be given to measurements rated good to excellent than to measurements rated fair to poor.`" Because the peak flow for 1972 was rated "fair", more weight should be given to the smaller annual peaks located on the shift curve of post-1990 points, so a linear extrapolation on logarithmic paper that bypasses the 1972 peak would be expected. This is especially true because any future flood at the gage that reaches a stage of 27.90 feet is all but guaranteed not to match the 1972 flow because of the reduction in cross section in 1990.

3) Rantz, page 359 - "`The effect of a change in channel width on the stage-discharge relation, unaccompanied by a change in streambed elevation, is to change the discharge, for a given gage height, by a fixed percentage. … The shift curve for a change in width alone will therefore plot on logarithmic graph paper as a straight line that is parallel to the original linear rating curve.`" Again, it appears that the rating curve should be based on the post-1990 flows, even though this would result in a linear extrapolation that misses the 1972 peak, however this is the expected result of the curve shift according to Rantz et.al.

The procedures recommended by Rantz, et.al. pretty much describes the procedure I used in the first part of my case study to obtain a flow for the 1972 annual peak of 58,600 cfs. Even if USGS staff disagree, I believe anyone using the gage should be aware that the pre-1990 data were obtained under different conditions.

Comment 2: I request that the January 4, 2017 decision be revised to indicate on page 1, paragraph 2 that the first reduction to 59,600 cfs was primarily based on eccentric flow, and that the further reduction to 50,300 cfs was based on other factors (please see my Comment 3 below).

The recent decision states correctly (p. 1, ¶ 2) that the initial estimate for the 1972 peak was 88,300 cfs, but then goes on to state that "The peak was later revised in 1990 to 50,300 cfs, primarily based on eccentric flow". I partially agree with this because the eccentric flow seems to have been a very important factor in the overall reduction. However, in his second paragraph Flippo (1990) only mentioned eccentric flow in regard to the first reduction from 88,300 cfs to 59,600 cfs. Other factors affecting the first flow reduction to 59,600 cfs were adjustment of the approach section, and bridge skew.

The factors that led to the second reduction to 50,300 cfs were a double peak, tailwater fall/slope, and Manning roughness in the downstream cross section(s).

I make this distinction because the first reduction for eccentric flow, adjustment of approach section, and bridge skew is based on quantifiable physical characteristics measured at the site, while the second reduction seems to be based more on Engineering judgement and factors that are more difficult to quantify.

Comment 3: For the 1972 annual peak, the first flow reduction from 88,300 cfs to 59,600 cfs (Flippo, 1990) is documented well enough and can be attributed mostly to eccentric flow, but the second reduction to 50,300 doesn't seem as well justified.

In the recent decision, USGS (p. 2, ¶ 4) addresses the 1990 flow reductions, listing most of the factors on which the reduction was based. Three of these factors (1-eccentric flow, 2-placement of the upstream cross section and subsequent parameters, and 3-bridge skew) were mentioned by Flippo in the second paragraph of his revision comments as the reasons for the *first* reduction from 88,300 cfs to 59,600 cfs. The remaining two factors listed by USGS (4-slope, 5-Manning roughness coefficient) were mentioned by Flippo in his third paragraph as reasons for the *second* reduction from 59,600 cfs to 50,300 cfs. At the beginning of Flippo's third paragraph, was an additional factor (6-double peak) which is not mentioned in the recent USGS decision.

Considering the last three factors, in the order listed;

(<u>Factor 4-Slope</u>) Flippo indicated that the original fall (used in the original and 1st revised flow estimates) was 1.09 feet. He also mentioned the FIS profile slope, which is a reference to the 1978 Roy F. Weston flood simulation study done in HEC-2, which I described in my case study. For the second flow revision to 50,300 cfs, Flippo indicated in paragraph 3 that the absolute maximum fall was 0.75 feet and may have been as low as 0.60 feet. In the same paragraph he mentioned the tailwater of 71.6, which only makes sense if he meant <u>2</u>71.6 (this is the tailwater I used in my case study). I can't follow what Flippo was doing with the slope revisions, but I do know that I used the same 1978 Roy F. Weston HEC-2 simulation in addition to a 2013 HEC-RAS simulation by Dewberry Inc. I was able to verify that the tailwater elevation of 271.6 produced the expected headwater at the gage, and I assume that having verified the tailwater, HEC-RAS would have taken care of the tailwater slopes from that point on in both the 1978 and 2013 simulations. For that reason it doesn't appear that tailwater slope should have been much of a factor in the second flow revision to 50,300 cfs.

(Factor 5-Manning roughness coefficient) In paragraph 3 Flippo described adjustment of the Manning coefficient to 0.036, and it seems he was referring to the the tailwater section(s). The 1978 HEC-2 simulation by Roy F. Weston initially used a Manning "n" of 0.04 in all the tailwater sections (Weaver, 2016). Assuming USGS used similar values in their analysis, reducing "n" to 0.036 would have increased the flow at the gage by 1,700 cfs, not reduced it. On the other hand, Design Charts for Open Channel Flow recommends a Manning "n" of 0.028 to 0.033 for major stream beds. If the Manning "n" was originally 0.032 and Flippo increased this to 0.036, there would be a reduction at the gage of about 1,900 cfs, reducing the flow from 59,600 cfs to 57,700 cfs. I note that Dewberry, Inc. used an "n" of 0.03 in all the cross sections downstream of the gage in their 2013 HEC-RAS simulation for the new FIS, however this may have been part of their attempt to calibrate their model to the flow for Agnes (which they did). The problem with this is Dewberry used the present day Viaduct cross section that includes East Walnut Street so their calibration is incorrect.

(Factor 6-Double Peak) In thinking about the double peak, listed first in Flippo's third paragraph, it isn't clear how this would have helped justify a 9,300 cfs reduction in flow, assuming the previous estimates (whether 88,300 or 59,600 cfs) would have been based on the overall highest water mark at stage 27.80 feet. If the second peak was actually 0.52 feet lower (stage 27.28 feet) as Flippo seems to indicate, HEC-RAS would produce a flow about 9,000 cfs lower (I checked), but Flippo actually proposed an increase in the high water mark from a stage of 27.80 to 27.90 (fourth paragraph of his 1990 comments). I may be misunderstanding the situation, but there doesn't seem to be any reason to even consider a second lower peak.

Comment 4: In my the original revision request on Feb. 10, 2016, I stated there was no direct effect on anyone due to what appeared to be an error in the flow for Agnes, because the FIS was based on Lancaster County model flows. I now believe that was a mistake and would like to correct my previous statement.

I know that Dewberry, Inc. calibrated the recently revised FEMA FIS to the high water mark at gage 01576500, but Dewberry, Inc. incorrectly used the existing cross section which includes East Walnut Street. It recently occurred to me that the FIS was probably calibrated to all the known high water marks for Agnes, of which there are eight along the length of the river, plus the one at the gage. If Dewberry Inc.'s HEC-RAS flood model was calibrated to high water marks assuming Agnes at 50,300 cfs (or the adjusted flow based on tributary area), but the flow for Agnes was actually greater, then the HEC-RAS model would overestimate the 100 year flood elevation at all points, not to mention the other return periods. This would have a large impact on anyone owning property along the river due to flood insurance requirements.

References:

Rantz S.E. et al. (1982). Measurement and computation of streamflow: Volume 2. Computation of Discharge. U.S. Geological Survey Water Supply Paper 2175, Washington, D.C. p. 346.

Weaver, A. (2016). "Reanalysis of a Flood of Record Using HEC-2, HEC-RAS, and USGS Gage Data." *J. Hydrol. Eng.*, DOI: 10.1061/(ASCE)HE.1943-5584.0001354.